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ABSTRACT

This paper discusses the process of culture acquisition and recommends a new methodology for investigating this process. An examination of the relationship between culture acquisition and cognitive development is followed by a discussion of the way previous research has dealt with this relationship. It is argued that recent developments in linguistics and psychology, as well as in anthropology motivate a reexamination of current assumptions concerning what constitutes a feasible and productive approach to the study of culture acquisition. A new approach is advocated in which culture acquisition is a life-long process of adaptation to one's social and physical environment which is punctuated by periods of relative congruence between one's current theory of reality and the information currently available about that reality. Some examples of how this approach might be applied are provided, using as data research by Williams and Danziger on children's use of kinship terminology, supplemented by some original research data on children's use of honorifics (terms denoting respect). Finally, a summary of the critical issues in the culture acquisition process is provided along with specific recommendations for future research on the topic. (Author/BH)

TOWARDS A COGNITIVE APPROACH TO THE STUDY OF CULTURE ACQUISITION

by

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ABSTRACT

This paper is concerned with the process of culture acquisition and, especially, with the way this process can be investigated. It begins by examining the relationship between culture acquisition and cognitive development, followed by a discussion of the way previous research has dealt with this relationship. Recent developments in linguistics and psychology, as well as in anthropology, it argues, motivate a reexamination of current assumptions concerning what constitutes a feasible and productive approach to the study of culture acquisition, and a new approach is advocated. It then provides some examples of how this approach might be applied, using as data research by Williams and Danziger on children's use of kinship terminology, supplemented by some original research data on children's use of honorifics. Finally, a summary of the critical issues in the culture acquisition process is provided along with specific recommendations for future research on the topic.

I. Culture Acquisition and Cognitive Development

In this paper, I will examine the following question: what is entailed in the process of culture acquisition, and what is an appropriate way to investigate this process? Any answer to this question must begin with some explanation of precisely what we can presume is being "acquired" in such a process, i.e., it must begin with some conceptualization of culture. What can be said about a term that has consistently aroused so much disagreement? At least two points can claim consensus: first, culture is something that must be learned; and second, whatever else may be intended by the term, culture involves some ordering of ideas about the world. Consequently, any account of culture acquisition must include some theory of learning. In addition, to the degree that culture is conceptualized as including ideational phenomena, i.e., cognitive systems of some sort, the acquisition of culture must be characterized as the development of these cognitive systems. This suggests that a description of this process will require, at least implicitly, some theory of cognitive development in light of which culture acquisition may be viewed as one very important aspect of an individual's overall cognitive growth.

How have previous researchers of the topic handled this learning process? Early attempts by anthropologists to describe certain aspects of culture acquisition known as "socialization" were greatly inspired by Freudian psychology. In the 1920's and 30's for example, the "culture-and-personality" school began to explore the effects of various child-rearing practices on group personality and sociocultural institutions (e.g., Kardiner, 1939; DuBois, 1944; Linton, 1945). Although the approach was initially applied only to small-scale societies, the exigencies of

World War II were such that their concepts of "basic" and "modal" personalities came to be applied with increasing frequency to more complex societies in the form of "national character" studies in the 1940's and 50's (e.g., Mead and Metraux, 1953; Hsu, 1953).

A second major impetus to this line of research was the "Yale learning theory," developed by Hull and his students, which explored the relationship between reinforcement and behavior patterns, how goals and drives can be modified, how habits can be formed. Using these notions, as well as a set of culturally modified psychoanalytic hypothesis, John Whiting's group at Harvard conducted a cross-cultural survey of child-rearing practices and cultural variables in an attempt to find reliable correlations between developmental experience and adult behavior (Whiting and Child, 1953). This same group also pursued more intensive case studies based on their own fieldwork (Whiting, 1963; Child and Lambert, 1966).

Although an impressive body of data has been amassed by these and similar studies (data most often in the form of significant statistical correlations), and although research of this nature continues today, a growing dissatisfaction with the fruitfulness of this approach is evident. As T. R. Williams has commented in a recent statement on the matter, "Despite a rapidly increasing volume of socialization research, cultural and social anthropologists appear to evince little interest in use of conclusions derived from such studies" (1978; 143). This lack of enthusiasm, Williams believes, is due to reluctance on the part of anthropologists to work with "decontextualized data," a reluctance he

sees as misplaced since "procedures in cross-cultural research, and particularly in socialization studies, do not decontextualize data any more seriously than the so called 'holistic' method of cultural and social analysis" and since statistical claims are only intended to be taken "as evidence for, rather than proof of, empirical validity."

With this Williams hints at but effectively avoids confronting the relevant issue in current methods of socialization research, viz., how are we to regard the innumerable statistical correlations available between various child-rearing techniques, and adult personality and cultural institutions? In an appendix to his article, Williams provides a list of 65 such correlations. Consider the following examples:

> Harsh parental treatment during infancy leads to cultural beliefs that the spirit world is harsh and aggressive (Spiro and D'Andrade 1958; Lambert, Triandis and Wolf 1959; Whiting 1959a).
>
> Stress during infancy leads to a wide range of adult song styles and polyphonyia [sic] ; lack of stress in infancy leads to adult monotonic singing (Ayres 1968).
>
> Societies with early weaning, early independence training and early training in modesty tend to explain illness as the responsibility of the patient (Whiting and Child 1953).

Presumably, the correlations these statements express are largely positive so that they deserve, in some sense, to be called "significant." But what does this really mean? It says, simply, that the liklihood of this fact being there due to chance is small, and it implies that the fact should follow from something else. However, one thing it most certainly does not indicate is that the relationship between the components of the correlation is necessarily one of casuality. In fact, a "significant correlation" says nothing whatsoever about the relationship

between its components; it is merely a fact in need of interpretation. Thus, the use of the words "leads to" in the first two examples is misleading at best. Similarly, in reference to the third example, no ture tendency of any sort has been demonstrated, and I suspect that the words "tend to" might be more appropriately replaced with "often."

Williams himself offers a suggestion for future research on socialization, based on Cohen (1964), in which "reasoned inferences" (derived from "repeated readings of entire ethnographic sources") are tested by an examination of "new cases" in the field. These inferences are themselves to be framed in terms of suspected correlations. He is quite optimistic about the results of such a procedure:

> Once the inferences...have been placed against a variety of 'new cases' of the widest possible cultural and social diversity, they will have been raised to the conceptual level of the greatest clarity, which is the last main division of all scientific discourse, that is, to the level of a conclusion, or a decision concerning the nature of an inference, or inferences.

But what nature of "conclusion" can such a procedure possibly yield? If the inferences being "tested" in this manner are themselves second-order correlations, then their confirmation gives us very little in the way of new insights of understanding. At most, it simply strengthens our hunch that, indeed, something interesting is happening here. What, precisely, this "something" is remains mysterious.

This, then, is one of the problems associated with the culture-and-personality school that has no doubt contributed to the general lack of interest in socialization studies which Williams and others have noted. Since the objective of the studies has most often been the compilation of yet additional "significant statistical correlations," we now find-

ourselves in the position of having gathered more "facts" than we know how to explain and under the distinct impression that we are making little or no progress.

But the culture-and-personality school has suffered from yet another, more serious problem; since both "personality" and "culture" were being described in terms of expected behavior patterns, how could one help but find correlations "between" personality and culture? In an effort to break out of this circularity, some researchers began to experiment with a different measure of personality through the use of "projective tests" such as the Rorschach inkblot plates and the TAT (Thematic Apperception Test), hoping that these provided a more reliable and objective index (Gladwin and Sarason, 1953; Wallace, 1952; G. D. Spindler, 1955; L. S. Spindler, 1962; Phillips, 1965). By the late 1960's, however, the validity of such tests as cross-cultural indications of personality faced increasing challenge. The possibility that culturally homogeneous responses to these tests reflected an application of cultural categroies and a pattern of perception (rather than some standardized personality type) had to be confronted.

Researchers continued to lose faith in the affect-oriented schema as it became increasingly apparent that such an approach failed to distinguish cultures adequately. Breakthroughs in other fields (Chomsky, 1957; Miller, Galanter and Pribram, 1960), fortunately, accompanied this loss of descriptive power and allowed a more productive approach to the problem. As a result, anthropologists began to give more attention to cognition (knowledge, beliefs, plans of action) and correspondingly less attention to affect (unconscious drives, "ego-satisfaction," etc.).

This interest in cognition has continued to the present time where it has provided the impetus for a number of different attempts at reformulating the culture concept, all of which give a much more prominent position to systems of knowledge and belief than previous formulations (e.g., Levi-Strauss, 1966; Blumer, 1969; Wallace, 1970; Goodenough, 1971; Keesing, 1972; Geertz, 1973). Of these various contributions to a cognition-oriented cultural anthropology, I believe those that carefully distinguish the organization of cognitive systems from the organization of actual behavior (in a manner analogous to the linguistic distinction between "competence" and "performance") offer the most descriptive power. Culture, according to this view, does not consist of observable behavior and events per se, but rather in the conceptual apparatus by which a people classifies, orders, and interprets such phenomena. Since this perspective has been gaining wide acceptance among anthropologists, and since the previously mentioned studies of socialization were guided by a rather different view of culture, it is appropriate to examine the implications of this latest formulation for subsequent research on culture acquisition. In particular, several questions arise as to how culture when viewed as a cognitive system might be acquired. For example, when (if ever) does a child's organization of knowledge and beliefs begin to resemble that of the adults in his society? Does this happen sooner and/or more often in some domains of culture than in others? Do a society's preferences regarding child-rearing techniques advance or retard the child's acquisition of "cultural competence," perhaps selectively? What is the connection between culture acquisition and culture change? In addition, since such an approach would be concerned with the acquisition

of cognitive structures (as opposed to gross behavioral responses), it would have to ally itself more explicitly than previous research with some theory of cognitive development that (unlike the Freudian and behaviorist accounts previously used) would be willing to confront head on the issue of how knowledge and belief is organized in human minds.

What do we have available in the way of candidates for such a theory? In the field of linguistics, considerable attention has been given to the relationship between language acquisition and cognitive development, but little attempt has been made to enlarge the scope of these accounts to address aspects of culture other than language. This has been the case even when the process of language acquisition is seen as being highly dependent on other, more general cognitive processes (Bever, 1970; Slobin, 1973). Nevertheless, linguistic formulations of language acquisition appear to have stimulated a significant shift in the way at least some anthropologists conceptualize the process of culture acquisition, as evidenced by the following remarks (each of which appeared as mere "asides" in the context of works dealing with other topics):

> ...acquisition is a matter, not of learning as such, but of constructing some set of theories about the world (competence) given an innate mechanism and a small finite input of data available to it. Thus, as situations change from one generation to another--that the range of observed behaviors from which a learner extrapolates a theory differs somewhat for each learner--so data inputs to the innate device change, and competence changes as well... (Durrenberger, 1971: 27-28).

> ...the human infant must be a theory builder of remarkable capacity. From a limited and imperfect sample of the possible events in a cultural universe, he or she must <u>create a theory</u> of the rules, programs, and logic of which this sample was an expression. The child must continually test and refine elements in this theory. For what social life requires is not an enactment of 'canned' sequences one has learned and stored.

> Rather, one must produce sequences one has never observed, but which are implied by one's theory. The situations of social life are ever-changing and often unique...they call for other behvaioral responses that are new to one's experience but culturally 'grammatical,' (Kessing, 1976: 202).

> What is wanted is replacement of a behaviorist paradigm of learning-as-conditioning with a Chomskian-cognitivist one, in which, from a fairly small input of affect, action, and precept observed, the human organism actively, even if unselfconsciously, constructs a conceptual representation of the world and of itself, (Lehman, 1972: 376).

These examples, brief as they are, give some indication of the truly radical turn that anthropological thinking on this matter has taken (at least among cognitivists). The legacy from Chomskian linguistics is clearly responsible for much of the current rethinking of the culture acquisition process, even though the specificity of the linguistic account of language acquistion makes it use as a model for culture acquisition untenable.

Within the field of developmental psychology, there is another, perhaps more promising candidate for a theory of cognitive development in terms of which culture acquisition might profitably be described. This is the work of Jean Piaget and his collaborators. Since the 1950's, Piaget's work has enjoyed renewed interest in the United States where it has been able to provide a viable alternative to the dominant trend in American child psychology, viz., the "stimulus-response," behaviorist tradition. Significantly, of all the theories of child development, Piaget's is the one most securely founded upon the study of the child (in contrast especially to Freud, Hull, and Skinner, who hardly studied children at all). His methods of observing and interviewing children are remarkably similar to those practiced by anthropologists. Most important, Piaget's

claims about the nature of human cognition are quite compatible with many of those that have been independently advanced by anthropologists. In particular, as a theory of the "structuralist-organismic" genre (Flavell, 1977), Piaget's account of cognitive development makes claims about both the nature of cognition and the way that cognition relates to its environment. According to his view, human cognition is specific form of biological adaptation of a complex organism to a complex environment. He posits the existence of cognitive structures ("schemas") within the mind that are organized so as to form a complex, dynamic system. This system can be modified as a result of interaction with its envionment ("accommodation") but it also serves to limit the way an individual will be able to interpret events within his environment ("assimilation"), thus allowing for systematic change due to experience without having to sacrifice all sense of contihuity.

In contrast to behaviorist models, then, the developing individual is portrayed as an active, internally motivated organism who participates in his own development, rather than as an essentially passive creature who is molded by an imposing environment. Interaction between the organism and his environment is stressed, the mind building an internal representation of reality by interpreting, transforming, and reorganizing environmental stimuli. Such a depiction is congruent with many important claims of contemporary anthropologists; it complements both the congitivists' assertions of underlying cognitive processes and the interactionists' contentions regarding the social construction of reality.

Two additional aspects of Piaget's theory are of special interest to those anthropologists who are concerned with the process of culture

acquisition. First, Piaget claims that the mental structures and operations characteristic of children's thought are qualitatively different from those found in adult thinking. That is, rather than suggesting that children simply understand less about a given concept or process, he claims that they understand it differently. A second and related point is that the transition from children's thinking to adult thought is believed to be a gradual process characterized by a series of relatively stable "crystallizations" of perspective that occur in particular sequences commonly referred to as "stages." These last two claims suggest that children would come to an understanding of their culture in some predictable sequence of stages; they challenge the anthropologist working in this domain to reveal the content and nature of that cultural sequence as well as the kind of experiences that are responsible for the transition from one level of understanding to another.

By integrating the insights of transformational linguists and developmental psychologists with the recent theoretical contributions of the so-called "cognitive anthropologists," a coherent picture of the process of culture acquisition begins to emerge. Cultural knowledge is perceived as some subset of one's total cognition, and therefore subject to the same constrains as any other type of human cognitive phenomenon. This means, among other things, that it is constructed by the individual through interaction with the environment and constitutes an internal representation of that environment. Its development is characterized by the recursive modification and subsequent stabilization of integrated cognitive systems, each of which provides the individual with a slightly different and increasingly complex "theory" about the nature of the world.

In addition, one suspects that the more complex such theories become, the more they will resist revision (an "inertia hypothesis"). The internal representations guide (but do not predetermine) an individual's behavior by ordering, interpreting, and assigning meaning to his perceptions of the environment.

In other words, I am suggesting that children use their every day experiences as, literally, "food for thought"; the information they obtain by interacting with people and things in their environment serve as <u>input</u> to a theory they construct about the nature of reality. This theory, in turn, serves as a sort of "working hypothesis" for future interactions; it helps them anticipate what people might do next, for example, and provides them with a framework on which they can "hang" or organize additional bits of information that come their way. Information for which no appropriate structure is currently available will either be forgotten or "understood" in terms of some other, inappropriate structure (Piaget's "assimilation"). When this happens, adults in the community will perhaps assert that the child "does not understand" or has "misbehaved." Of course, if one encounters any truly novel experiences that one's current theory of the world simply cannot handle, one will have to reorganize one's ideas and modify or revise the structure of one's cognitive system so that the new experience <u>can</u> be understood (Piaget's "accomodation"). If, for some reason, this is impossible, that experience will be denied or overlooked. Culture acquisition, then, is seen as a life-long process of creative adaptation to one's social and physical environment that is punctuated by periods of more or less "clarity of vision"--a situation brought about by the relative degree of "fit" (congruence) between one's current theory of reality and the information currently available about that reality.

II. The Application of This Approach to Cultural Data

What are the methodological consequences of conceptualizing the process of culture acquisition in the manner just described? For one thing, it means that the end result of one's research will take the form of a description of mental states rather than a description of behavior, i.e., a description of "competence" rather than "performance." One advantage of this is that the former is, of necessity, more systematically organized than the latter. As Durrenberger has stated:

> One further reason to deal with competence rather than performance is, as Chomsky argues (Chomsky, 1957), that it is possible to describe competence but not behavior in a consistent and enlightening manner. This is not to say that competence is somehow remote from behavior, that it is alien to it. Indeed, it is just the power of a description of competence to capture generalizations about behavior which cannot otherwise be unified that motivates such an approach. Without some account of competence, for instance, there can be no account of mistakes since they are interpreted as such by reference to competence, (op. cit.: 26).

I would like to pursue Durrenberger's last statement somewhat by examining two studies, one by Brett Williams (1972) and one by K. Danziger (1957), that are concerned with the process whereby an individual learns to use the kinship terminology of his society appropriately. These two studies are particularly useful for my purposes since each contains a generous amount of first-hand data in the form of literal transcriptions of actual conversations with children. I will discuss each in turn, indicating whenever appropriate those aspects of their data that might be further elucidated by reference to the possible theories of this domain that children might construct.

In "Children's Kin," Williams attempts to analyze a body of data she has collected in terms of the theory of kinship proposed by Lehman

and Witz. Only the most fundamental arguments of this theory will be necessary to the discussion of this paper. These come in the form of a model with three levels: (1) "G" for "genealogical claims"--this is the set of all possible genealogical claims (a claim being anything of the form "x's father is y" or "y's mother's sister is z"), (2) "PGS" for "primary genealogical space"--this is the result of a series of mathematical operations that transform simple claim strings in G into positions in a relational "space." The structure of this space (imposed by the PGS rules) is said to delimit the patterns of thought which connect positions in it, (3) "K" for "kinship terms" (labelled categories)--this is the level that varies from culture to culture, the level where the infinite set of PGS positions are "lumped" into a finite number of jural kin categories onto which lexical "kin terms" are mapped (Gatewood, 1972).

What is available to the child, then, is data solely from the level of K. By providing examples from her interviews, Williams is able to demonstrate that children do indeed treat this information from K as evidence for some kind of underlying system of rules and, eventually, are able to conceptualize this system as being PGS. According to Williams, this transition from limited ability in K to mastery of PGS is the result of the child's gradually increasing ability to perform certain logical manipulations or "integrations" that are crucial for operating in PGS. The argument is that, since the underlying rules of kinship terms are "purely formal"--in the sense that they are based on formal rather than behavioral feature specifications--the child's increasing ability to use the terms appropriately is a direct reflection of his increasing ability to maneuver in formal systems. Thus, when Williams explains

children's errors in the use of comprehension of kin terms as, for example, "their inability to compute filial links through time" (p. 24), she is offering an explanation that correlates children's cultural competence with their overall cognitive development. In doing so, emphasis is placed on thought processes, an appropriate concern for an analysis such as this. However, the analysis might proceed even further if the content of the children's thought was given similar consideration, i.e., if attention was given not only to how the children were thinking but to what they were thinking as well.

Consider, for example, the following exchanges with Williams' informant "Cathy," age 4:

B: He's your grandpa...
C: Yeah.
B: Is he anybody's daddy?
C: Yeah, he's my grandma's daddy...
B: Does your Mommy have a Daddy?
C: Yes he's at class.
B: Your daddy's at his class...

Williams considers these responses to be an indication either of logical error or of an ambiguity concerning what it means to "have" a certain relative. This latter explanation, I believe, is more to the point, but is not taken far enough. It is quite possible that Cathy has formulated some hypothesis as to the meaning of the kin term "daddy" that makes it roughly equivalent to "male head of household"; this is certainly a reasonable hypothesis in any case, and could easily account for much data before it would have to be rejected. Notice, too, that such a hypothesis, even though "incorrect," allows logical manipulations that are as sophisticated as any that can be applied to the "correct" one. (It would allow Cathy to see, for example, that her daddy is not everyone's daddy, or that a daddy can also be an uncle--although not to the same person, etc.)

Another example involves Heather (age 6) and her use of the term "children":

B: How did they get to be your grandpa and grandma?
H: They were little kids, then they grew up to be Mommies and Daddies like my Mommy and Daddy and then they're Grandmas and Grandpas.
B: Did they have any children?...
H: No....

B: When your mother's a grandma, who will be her grandchildren?
H: Us.
B: You?
H: Yeah.
B: I thought you were her children.
H: Uh, grandchildren will be our grandchildren...our children.

Williams explains the change in attitude exhibited in the last line by suggesting that Heather (and her sister Jenny, not shown here),

> when confronted with their own contradictions, begin to figure something out. Although Heather at first claims that her mother's grandchildren will be her and Jenny, she realizes when prodded that that relationship cannot change and she formulates a new and correct string.

There is another explanation, however, and that is that Heather does not use the word "children" as a kin term, and perhaps does not recognize it as such until the distinction is made by Williams between "children" and "grandchildren" in the next to last line of the exchange. Prior to that, Heather could just as easily have been using the word to mean "non-adult who lives in the house of," whereas Williams was using it as the kin term meaning "son or daughter of." This becomes even more plausible in a later part of the same conversation:

B: Does your grandma have any other children?
H: Just grandchildren, that's us, and she has six grandchildren.
B: How about her children? Does she have a son?
H: No--just six grandchildren.
B: But you said that she was your aunt's mother.
H: She is. Because when my aunt was her children when she was marired.

Williams' explanation is still conceivable, of course, but the point is, as long as a child's errors can be accounted for in some other, equally plausible way, the possibility remains that he has simply not yet received sufficient input to tip the balance of evidence in favor of the correct theory over the one he is currently using. If so, the child's mistakes are not due to logical errors as such, but rather to a simple lack of crucial information.

Notice that these two explanations are not mutually exclusive. In all likelihood, the child's mistakes in the use of kinship terms are due to differences in both the processes and the content of his thought from those of adults in his community. I have called attention to the latter simply because it is the most often neglected and also because in the domain of kinship, it is particularly well-motivated. The fact that kinship systems are organized on the basis of wholly formal criteria (i.e., genealogical information) makes them unusual. Few domains of human experience exhibit this property (some, in fact, would argue that kinship is unique in this regard) and there is no reason to suppose that this property would be among a child's "first guesses" regarding the nature of the system's underlying rules. This especially would be the case if the child had been exposed to some other domain of his culture and had constructed a successful theory of that domain without attributing to it an empirical basis that was purely formal. In this case (and, perhaps, at any rate), we could expect him to attempt to build a theory of the kinship system by relying primarily on certain behavioral specifications that appeared to him to be particularly salient. If imaginatively constructed, a theory based on such specifications could easily prove

adequate for a large number of cases. In fact, a considerable amount of data of varying kinds would have to be available to the child before he would be forced to conclude that the relevant information consisted solely of circumstances surrounding birth and marriage. It is perhaps even likely that such a conclusion would be the last one he would come to, simply because it is the least obvious.

Similar comments could be made about Danziger's study, "The Child's Understanding of Kinship Terms: a Study in the Development of Relational Concepts." The basic assumptions of this study are found in the opening paragraph.

> It is well known that young children do not use kinship terms correctly. A correct use of these terms implies that the child is able to handle relational judgments. But this ability does not appear until intellectual development has progressed a considerable way. The study of the development in the understanding of kinship terms can therefore form a useful approach to the problems associated with the transition from non-relational to relational thinking.

Although Danziger is undoubtedly right to assume that the correct use of kinship terms reveals much about the child's level of cognitive development, the interpretation of incorrect usage is still ambiguous; it remains an empirical issue whether to attribute this to a lack of capacity or a lack of critical input. Again, an example from the study will help to clarify the danger.

In an attempt to obtain definitions of kin terms from his informants, Danziger used the eliciting frame, "What is a ____?" (filling in the blank with terms like "brother," "cousin," etc.). This opening query was followed by others, where appropriate, that were intended to aid in clarifying the child's response (e.g., "How do you know?" or "What makes

you say x is a brother?"). Even with these additional questions, however, I do not think Danziger is justified in concluding that the answers he so obtained constitute actual, functioning definitions for the children. That is, the fact that a child responds to the question, "What is a brother?" with the answer, "a boy" does not appear to be sufficient cause to label that response "categorical," as Danziger does, and claim that it indicates a certain stage in the child's cognitive development. It is equally possible that the child has simply interpreted the word "is" in the interviewer's question to mean "is a member of the set that includes," rather than "is the defining characteristic of."

Both Danziger and Williams have tried to account for children's use and, in particular, their "misuse," of kin terms by making hypotheses about the children's reasoning processes. This, I believe, is a productive approach to take. In doing this, however, consideration should be given to both process and content. Concern for the latter inevitably leads to an interest in the process of language acquisition, since it require one to arrive at some hypothesis about what a child means when he says what he says. The acquisition of competence in the use of kinship terminology is particularly troublesome in this regard since, in many societies, such terms are often used to express not only kinship relations but also a host of other social relationships. These frequently, but not always, bear a metaphoric relationship to their usage in the kinship domain as in, for example, the widespread use of kin terms in a system of honorifics. Here, the meaning of a term depends on the context in which it is used, and thus, competence in the use of kinship terminology of necessity entails a recognition of various social contexts.

The previously mentioned example from Williams' study involving Heather's use of the term "children" illustrates the sort of systematic ambiguity with which I am concerned. In English, there is no specific kin term that means "son or daughter irrespective of sex" (i.e., the equivalent of 'sibling' for first generation descendents); the notion is most commonly expressed by using the word "child" or "children." However, these words in English are also used to refer to "non-adults," regardless of kin relationship. As a result, proper use of the term involves understanding which meaning is appropriate in any given circumstance. As I argued earlier, I believe Heather's "errors" in using the term were motivated by a failure to appropriately identify the context in which the term was being used. That is, she persisted in using "children" to mean "non-adult" until forced by Williams to redefine the context.

The relationship of this sort of dual use of kinship terms to the process of language acquisition becomes even more apparent when such terms are used in a system of honorific titles or terms of address. While engaged in exploratory fieldwork in northern Thailand, I had the opportunity to begin preliminary research on this topic in a small Shan village of approximately 160 people. Here, a combination of kin terms and a few other expressions are used as a prefix to proper names in order to denote status relationships among individuals, both as terms of address and as terms of reference. The most general relationship expressed in this manner is that of a younger person to an older one, a relationship characterized by respect and deference. Kin terms indicative of ascendent generations ("mother," "uncle," etc.) are used by the speaker to express the high status of the individual addressed or referred to relative to the speaker

himself, and vice versa. Typically, though not invariably, the honorifics used by any two individuals in reference to each other will closely correspond to their respective generations anyway, i.e., a person will refer to most adults in his parents' generation as "aunt" and "uncle," most of those in his grandparents' generation as "grandmother" and "grandfather," and so on. This is not to suggest that the honorifics employed by any two individuals are wholly predictable given their relative ages. The use of combinations of kin terms with other expressions (e.g., paw long, or "respected elder male") provides at least the potential for subtle distinctions, as does the fact that there often exists more than one term with the "same" meaning (e.g., puu and paw thau can both be glossed as "grandfather"). In addition, two kin terms are sometimes used in combination (e.g., paw lung, "father-uncle"). Thus, it is a system of some complexity that allows the speaker a certain degree of latitude in the choice of an honorific; for any given individual there will always be at least two or three appropriate terms from which to choose.

In order to make an initial assessment of the children's competence in this system, I elicited samples of their use of honorifics for people whom they knew by asking 16 children (9 boys and 7 girls) of various ages to identify people in a set of photographs that had been taken in their village. Of the 32 people who appeared in the photos, 17 were male and 15 were female; most were adults, but a few were children or teenagers. The instructions for each child were the same. I told them that I was having trouble remembering the names of all the people in the village and then asked for their help in identifying the photographs. Fortunately, this proved to be a task of considerable interest to the children so

that volunteers were not hard to come by. (The photographs, in fact, retained their attractiveness long after this particular project had ended.) As the children identified the various individuals, the honorific they used for each one was recorded.

Using this method, it was possible to identify a "core" group of about 12 different terms most frequently used by these children. Some of these are "redundant" (in the sense that they can be used interchangeably for the same person). Although most of these honorifics served to distinguish people according to generation and sex, a few are based on behavioral specifications such as puu, which appears to be used only in conjunction with the titles of certain offices (e.g., puu kan, "headman," or puu thien, "tambon head"), and sang, which is restricted to men who, at some point in their lives, have spent time as a novice in a Buddhist monastery. This core group of terms was being used in an appropriate fashion by all of the children in the sample, including some who were only 4 or 5 years old, indicating an ability on their part to make most of the significant distinctions between "types of people" at a remarkably young age. In fact, I tried and failed to find a child who was unable to make these distinctions, such children apparently belonging to that group that was too young to be able to talk to me.

The possibility remained, however, that the younger children had not really acquired competence in a system of honorifics but had, instead, simply learned the honorifics as part of a person's name. If so, the child would be using the appropriate term for a given person not because he recognized that individual as being a member of a particular social category, but rather because he considered the appropriate honorific to

be part of the person's name, To investigate this possibility, I repeated the photo identification task with half of the children in the original sample (4 boys and 4 girls), aged 4 to 11. In each case, their choice of honorifics for at least one of the photographed individuals was different from the choice they had made previously, indicating that they did indeed distinguish between the honorific and the name itself. Thus, we must include that these children have, by the time they have learned to speak, already made significant advances toward cultural competence: they have learned to distinguish several important categories of social identities and have acquired a modest repetoire of honorifics which they can appropriately use to express their recognition of these categories. Whether they have, in addition, learned to distinguish between the meaning kin terms have when used as honorifics and the meaning they have when used to express genealogical relationships is still unclear. On more than one occasion, a child identified an individual as "mother of x," "y's son," etc. However, since it is possible that these usages represent teknonymy and not a capacity for genealogical reckoning, the extent of the children's competence in the use of kin terms remains ambiguous.

What significance do these findings have by way of comparison with the research discussed earlier by Williams and Danziger, which also dealt with children's use of kinship terminology? One observation that can be made is that, while very young children (4 year olds) where shown capable of using kin terms successfully as part of a system of honorifics, children somewhat older had difficulties when asked to use the same (or equivalent) terms to express genealogical relationships. This is

problematical only if it is assumed that the task at hand is to learn how to use kin terms appropriately, irrespective of where they appear.

At this point, however, it should be apparent that there are actually two very different domains of competence involved here, both of which happen to inform the use of kin terms. The apparent discrepancy is due to the fact that mastery of an honorifics system requires a different set of cognitive skills than does mastery of a system of genealogical reckoning, even though successful performance in both domains involves the use of kin terms. Specifically, genealogical reckoning requires a capacity for relational logic. In contrast, competence in a system of honorifics appears to be related, at least in part, to language acquisition in that, by the time a child is able to engage in meaningful conversation, he has already acquired a significant degree of facility in this domain.

Of course, these claims are based on research findings that are quite tentative; the material on honorifics was the result of research that was strictly exploratory as, I believe, was the material in Williams' article. Consequently, the data may prove to be misleading and the claims they suggest may be altogether wrong. However, it is not the findings themselves to which I wish to draw attention. My purpose in ths section has simply been to demonstrate that interesting information can be obtained by an approach to culture acquisition that is oriented toward cognition and that seeks an interpretation of the data that credits the growing individual with some motivated, systematic theory of his envoironment.

III. Towards a Cognitive Approach to Culture Acquisition

Throughout this paper, I have tried to point out those areas of concern neglected by previous research which merit our attention in the future. Several issues have emerged. First, if we are going to use what we know about children's cultural performance (or "output") to make claims about their cognitive capacities, it is absolutely crucial that we pay more attention to input, i.e., we must have a much better idea of the nature and extent of information available to a child on any given subject before we can adequately assess his reasoning about that subject. This, of course, will require long and intense periods of naturalistic observation and "sleuthing"; the necessary information simply cannot be obtained in any other way. Countless hours of following children around and recording what they do and, especially, what attracts their attention, has convinced me that this kind of data is essential to have, if tedious to collect.

Second, we must be more alert to the context-sensitive nature of children's behavior and, accordingly, more cautious in our generalizations about the cognitive processes that presumably underlie this behavior. As I have tried to demonstrate in the previous section, many kinds of behavior (such as the use of kin terms) are appropriate in more than one context. Hence, adequate performance in one context does not presume adquate performance in the others since the underlying "rules" informing the behavior in each context may be quite different. "Context" here refers both to temporally bounded events (such as religious ceremony or village meeting) and to topically bounded domains (such as agricultural decisions or genealogical reckoning).

Third, analysis of culturally approved behaviors and social skills in terms of the cognitive tasks they require and represent would be helpful. Such a breakdown of cultural performance into its cognitive prerequisites will hopefully allow us to see the relationships that hold among seemingly unconnected behaviors (e.g., that behavior A is dependent upon mastery of skills B and C, etc.).

Essentially, these issues reflect the problem of descriptive adequacy; we do not yet have sufficiently rich descriptions of children's situated use of their cognitive capacities to allow a consistent and productive system of explanation. Any descriptive ethnography that claims to represent the chidlren's perspective should be able to provide answers to questions like the following: What is the range of social roles that children of various ages recognize and where do they place themselves among these? What kinds of differences in social context are recognized by children, and does the criteria by which they discriminate these contexts (such as, perhaps, "presence or absence of adults") differ from those used by adults? What do the children consider to be appropriate behavior for these various situations? When do they begin to allow for differences in behavior according to perceived differences in role?

Implicit in all of these recommendations is a conviction that we should not be content to discern and record the changing patterns of behavior that accompany an individual's growth and development. We must, in addition, be willing to speculate about the patterns of thought of which these behaviors are an expression and from which they derive their meaning. This need not be a dangerous enterprise. As long as we speculate in the form of testable hypotheses, i.e., hypotheses with empirical implications,

our understanding of the process of culture acquisition will progress. As I have argued in the preceding pages, I believe the most productive hypotheses will be those that emphasize the content and nature of children's emerging sense of social structure from the perspective of a broader theory of cognitive development.

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